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THE STIMULUS-ERROR*

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The purpose of this paper is to discuss the "stimulus-error," to indicate something of its history (though limits of space will preclude more than a bare outline), to add something by way of definition (since definition has remained implicit and there are some who do not understand this term), to enquire, at the level of the scientific experiment, into the significance of the attitude which is thus styled an "error" (relying as much upon experimental observation and as little upon epistemological conviction as is possible), and to arrive, if may be, at an evaluation of the stimulus-error or stimulus-attitude in its relation to the psychology of the present day. This is not so large an order that it does not need filling. Some psychologists put out of court experiments that involve the stimulus-error; others refuse to see any 'error' at all and discount the works that stress this 'merely epistemological' distinction. And when we seek a sanction for the one view or the other, we are at a loss whither to turn, for the "stimulus-error," although it has a long history, has been left to make its way without any very formal introduction.

* The present paper is the outcome of a promise to deal specifically with the nature of the stimulus-error, especially with its relation to psychological measurement and psychophysics. Cf. the discussions of the present writer in *The logic of the normal law of error in mental measurement*, *Amer. J. Psychol.*, 1920, 31, 1-33, esp. 27ff.; and in *The control of attitude in psychophysical experiments*, *Psychol. Review*, 1920, 27, 440-452, esp. 447f., 449 and note.

The Stimulus-Error

Undoubtedly much of the confusion and disagreement has been brought about by the term itself: "stimulus-error." It implies something that is right and something that is wrong, defending one position and impugning another. It serves, and was intended, to throw two positions into contrast, to insist upon an important distinction that is often overlooked; yet does not stop with definition, but goes on to pass a judgment. In this dual function of the phrase there has been both an advantage and a disadvantage. To those who accept both the implied distinction and the explicit evaluation the notion has been exceedingly useful, for it has enabled them, not only to separate the methodological sheep from the goats, but also to dispense with the goats,—a telescoping of procedure that is convenient and economical. On the other hand, those psychologists who have staked their fortunes on the goats of stimulus are not to be reformed by being found in the way of the "stimulus-error." They simply deny the "error" and in so doing miss the more fundamental distinction between opposing positions that must be made out before judgment can be passed upon either. We ourselves must not be thus misled, whatever our ultimate judgment may be.

This implied opposition, which we must now bear clearly in mind, is the fundamental opposition in psychology—or between psychologies—of mental process and meaning, of content and object, of *Beschreibung* and *Kundgabe*. Titchener, who is responsible for the term "stimulus-error," puts the case thus:

"We are constantly confusing sensations with their stimuli, with their objects, with their meanings. Or rather—since the sensation of psychology has no object or meaning—we are constantly confusing logical abstraction with psychological analysis; we abstract a certain aspect of an object or meaning, and then treat this aspect as if it were a simple mental process, an element in the mental representation of the object or meaning. . . . We do not say, in ordinary conversation, that this visual sensation is lighter than that, but that this pair of gloves or this kind of grey paper is lighter than this other. We do not say that this complex of cutaneous or organic sensations is more intensive than that, but that this box or package is heavier than this other. We do not even say, as a rule, that this tonal quality is lower than that, but rather that this instrument is flat and must be tuned up to this other. Always in what we say there is a reference to the objects, to the meaning of the conscious complex. It is not the grey, pressure, tone, that we are thinking of; but the grey of leather or paper, the pressure of the box, the pitch of the violin. . . . What is more natural than to read the character of the stimuli, of the objects, into the 'sensations' with which certain aspects of the stimu-

lus or object are correlated? . . . This is what Fechner did. . . . [He] transferred to sensation a point of view that is right for stimulus, but that introspection refuses to recognize in psychology."¹

We commit the stimulus-error if we base our psychological reports upon objects rather than upon the mental material itself, or if, in the psycho-physical experiment, we make judgments of the stimulus and not judgments of sensation. At the more complex levels we may make a similar error, a 'meaning-error,' which consists of describing objects, reporting meanings, stating *Kundgabe*, instead of describing mental process or giving *Beschreibung*. We can not, however, in this paper, extend the discussion to include this complex level, but must content ourselves with the conviction that whatever applies in the controversy between judgment of stimulus and judgment of sensation, applies also to introspection and its rival, the statement of meaning. We may concede that the psychophysical experiment in its simplicity represents the ideal ultimate in the psychological experiment, where control of conditions and adequacy of observation are maximal; and that we should be glad to reduce all psychological observation to this degree of rigor at least. At any rate any extension of this discussion to the 'higher' processes must wait, for the history and application of the stimulus-error are at the level of psychophysics, and the interpretation of the "stimulus-error," up to which we are leading, shows most clearly here.

The Quantity Objection and the Stimulus-Error

It is not surprising that a psychophysics, which seeks to establish the relation between the mental and the physical, should emphasize the distinction between sensation and stimulus. What is surprising is that the opponents of psychophysics should have raised this very distinction for the confounding of psychophysics and should have claimed that the psychophysical relationship (the logarithmic relation of the Weber-Fechner law) was an artifact created, not by the attempt of the psychophysicist to distinguish between sensation and

¹ E. B. Titchener, *Experimental Psychology*, II, i, 1905, p. xxvif.; cf. *Text-book of Psychology*, 1910, 202f. Titchener first uses the term stimulus-error = "R-error" in *Exper. Psychol.*, II, ii, 1905; see pp. lxiii, 198f., 203ff., 207, 219, 223, 230f., 262, 450. For Titchener's further use of the term, see *Text-book*, pp. 218, 350, 398 note, 522. J. v. Kries characterized the objectifying attitude as a "source of error": "Wenn man aber diese Quelle des Irrthums ausschliesst und möglichst an den objectiven, als Reiz dienenden, Vorgang gar nicht denkt . . . ;" *Vtjschr. f. wiss. Philos.*, 1882, 6, 275; and Titchener seems to refer to this discussion as a sanction for the term "stimulus-error."

stimulus, but by his confusion of the two. Yet such is the substance of the "quantity objection" to psychophysics, which had later to be met with the psychological sense-distance by Müller, Titchener, and others, who thus turned the tables and brought the argument for the distinction between mental and physical material to the support of a Fechnerian psychophysics. This was a long and tedious battle, and one might have expected that the resultant emphasis upon the two-fold nature of psychophysics would have determined the psychophysical universe for a time. On the contrary, however, the confusion between sensation and stimulus persisted. Cattell was fathering a psychology of the stimulus, and it was in the tradition of the work of Fullerton and Cattell that Urban did his experiments. Now that behaviorism has come into vogue, it is not apparent that we do not have two kinds of psychophysics—a psychophysics of process that gives, as Fechner wanted, the correlation between mental and physical data, and a psychophysics of behavior that seeks to identify response with its stimulus. That this psychophysics of stimulus-and-response needs also, if it is to be scientific, to take account of the error that has been called the "stimulus-error" is the thesis of the present paper; but the thesis must wait upon the perspective of the preface.

A clear recognition of the distinction between mind and body, between consciousness and objects, was the key-note of Fechner's position. There was for him at least this dualism in the universe, which may be regarded from one standpoint or the other. The case is not unlike, Fechner argued, the Ptolemaic and Copernican worlds. The geocentric and heliocentric solar systems are different systems, and we may at pleasure take either point of view that we choose. The worlds remain distinct. Or the matter is like a circle, which may be viewed from the inside or the outside. In the one case we see only concavity, in the other only convexity. Such a dualism can be resolved only by the law of relationship that holds between its two aspects, and, just as the relationship between concavity and convexity can be stated geometrically for the circle, so the logarithmic law resolves the dualism of mind and body. There is no doubt, therefore, that Fechnerian psychophysics stands or falls according to its success in distinguishing between measurements of mind and measurements of body, or between sensation and the object of sensation, the stimulus.²

² G. Th. Fechner, *Elemente der Psychophysik*, esp. 1889, I, 1-12.

The vigorous opposition that developed to Fechner's psychophysics took its stand firmly upon the distinction between mind and body, but denied the possibility of a quantitative correlation between the two on the ground that mind was not possessed of magnitude and that mental measurement was an impossibility. This argument came to be known as the "quantity objection" and was the main source of opposition to quantitative psychology in the eighties and nineties of last century. Introspection, the objection runs, does not show that a sensation of great magnitude ever contains other sensations of lesser magnitude in the way that a heavy weight may [supposedly] be made up of a number of smaller weights. "Our feeling of pink," said James, "is surely not a portion of our feeling of scarlet; nor does the light of an electric arc seem to contain that of a tallow-candle in itself."³ "This sensation of 'gray,'" remarked Külpe, "is not two or three of that other sensation of 'gray.'"⁴ "A blue surface," Ebbinghaus commented, "is something other than a green, but the latter has in itself, apart from memory of the colors, nothing of the doubleness or threefoldness of the green. . . . A low tone sounds different from a high tone, and in like manner a loud tone different from a soft."⁵ In other words increase of magnitude in no sense means increase of complexity. A sensation is just itself no matter what its degree. The tone produced by many instruments in unison is not of itself composed of more units than is the tone from a single string, nor is the tone of many vibrations per second more complex than the tone of few vibrations. In this form the objection seems obvious enough. Sensational magnitude is certainly not multitude, and intense sensations are not integrated of more sensory stuff than are weak.⁶ How then was psychophysics to defend itself?

Its immediate defense was a display of the factual material. Here were the experimental measurements. If they were not observations of the magnitudes of sensation, what were they?

³ W. James, *Principles of Psychology*, 1890, I, 546.

⁴ O. Külpe, *Outlines of Psychology*, tr. 1896, 45.

⁵ H. Ebbinghaus, *Z. f. Psychol.*, 1890, 1, 323.

⁶ Titchener mentions as raising the quantity objection: G. E. Müller in 1878, Exner in 1879, Stadler in 1880, Zeller in 1881, Boas and F. A. Müller in 1882, Stumpf in 1883, Tannery in 1884, Elsas in 1886, Grotenfelt in 1888, James, Münsterberg, and Ebbinghaus in 1890, Sully in 1892, Külpe in 1893, Wahle in 1894, Meinong in 1896, Höfler in 1897, and Lehman in 1902. See Titchener, *Exper. Psychol.*, II, ii, pp. xlviii-lxiii.

To this question the raisers of the "quantity objection" replied that psychophysicists had created an artificial mental magnitude by a confusion of the sensation with the stimulus, that is to say, they had committed in their experimental work the "stimulus-error." This was a serious charge against a discipline that depended for its existence upon a sharp distinction between the mental and the physical. Let us see how the accusers dared to raise it.

Von Kries put the matter clearly:

"An illusion is thus very easily brought about by the fact that one tends in general to estimate objective values (measurable in objective terms) according to the sensation. If one, however, excludes this source of error, and in so far as possible thinks not at all of the objective process serving as a stimulus, then one must necessarily admit that a quantitative relation does not exist between the different parts of an intensive series. This fact is most obvious to us when we do not attempt objectification as, *e.g.*, in pain. Whatever it is called, a pain exactly ten times as strong as another does not admit of such absolute statement."⁷

Ebbinghaus, somewhat later, was even more explicit:

"In general one designates the brightness of a flame or a surface as 10 or 12 times another brightness, and could just as easily, it appears, designate a loud tone as the double or treble of a soft tone. But what occurs here is no longer an immediate sensation or an immediate judgment of sensations, but depends upon the introduction of experiences. We can readily experience, and we do every day experience, the fact that the arousal of a brightness or a loudness depends upon a diversity of just those physical things or processes that in limited number call forth the impression of darker or softer. In order to have an impression of greater brightness for a surface, one can increase the number of gas-flames illuminating it; in order to strengthen a tone, one multiplies the instruments carrying it. Such experiences with respect to the causes of sensations we have always in immediate view, and we believe that we have the numerical characteristics that always attach to the one occurring without anything further in the other. It is psychologically difficult to get rid of them, just as it is difficult not immediately to see in a grass-green apple its sourness. But if one succeeds in the perfectly possible separation of the thought context, then it is clear that, as the bare visual impression of an apple has no sourness in it, similarly the bare impression of brightness does not consist of the multiplicity of candles upon which, of course, it frequently depends."⁸

We have already seen what fifteen years later, Titchener had to say in the same vein and how, although defending mental measurement, he makes the charge of the stimulus-error against Fechner. And there were many others.

⁷ J. v. Kries, *loc. cit.*

⁸ H. Ebbinghaus, *op. cit.*, 323f.; cf. *Grundzüge der Psychologie*, I, 1905, 71-79.

Exner put forward the general argument in 1879;⁹ and Boas in 1882.¹⁰ Tannery said in 1883: "It is the objective study of the excitation and its variations that leads to this definition of number that measures the sensation. At bottom it is by excitation that sensation is defined."¹¹ On epistemological grounds both F. A. Müller¹² and Meinong¹³ concluded that mental magnitudes, unlike physical, were indivisible. And long before any of these, Brentano, the father of modern intentional psychology, had said: "If one measures, as Fechner did, the intensities of colors, tones, *etc.*, then one is measuring the intensities of physical phenomena. The color is not the seeing, the tone is not the hearing, the warmth is not the sensing of warmth."¹⁴

Nevertheless this still seems a surprising charge to bring against Fechnerian psychophysics. If the fundamental task of psychophysics is the discovery of the relationship between the hitherto unrelated body and mind, is it not astonishing that psychophysics should have confused the two, the two whose very separateness was the *raison d'être* of psychophysics? Yet the critics stuck to the point and were at pains to show the readiness with which these incommensurables did duty, the one for the other. Ward, pointing out that the psychophysical limen expressed in terms of stimulus was physical quantity, concluded: "There is no trespass harder to avoid than that across the lines dividing the subjective and objective aspects, and none more disastrous to the offender."¹⁵ Other writers urged the same point, and Külpe even brought the prevalence of objectification into an experimental study.¹⁶ It is no wonder then that objectification was thought of as a source of error and that Titchener coined for it the term "stimulus-error."

The Answer to the Quantity Objection

The fundamental and final answer to the quantity objection was Weber's law: $S = k \log R$. In so far as the relationship had been observed, no amount of explaining could explain it entirely away. It might be that the function was not exactly

⁹ S. Exner, Hermann's *Handbuch der Physiologie*, 1879, II, ii, 242.

¹⁰ F. Boas, *Pflüger's Arch.*, 1882, 28, 568f.

¹¹ J. Tannery in J. Delboeuf, *Éléments de psychophysique*, 1883, 138.

¹² F. A. Müller, *Das Axiom der Psychophysik*, 1882, 46-56.

¹³ A. Meinong, *Z. f. Psychol.*, 1896, 11, 81-133, esp. 96ff.

¹⁴ F. Brentano, *Psychologie vom empirischen Standpunkte*, 1874, I, 91.

¹⁵ J. Ward, *Mind*, 1876, O. S. 1, 460.

¹⁶ O. Külpe, Ueber die Objectivirung und Subjectivirung von Sinesindrücken, *Philos. Stud.*, 1902, 19, 508-556.

logarithmic or that it held only within certain limits; it might not be certain just what was the nature of S, or of R; but the unescapable fact was that there were an S and an R, which were covariant, and which were not identical since the mode of variation of the one was not the mode of variation of the other. To charge the stimulus-error and say that S was contaminated by R was not enough, since the confusion of S with R was not enough to explain the discovery of this difference in variation. The psychophysicists, therefore, had the stronger position, and had only to show where the difference actually lay. There seem to be five ways of accounting for the difference and thus of establishing psychophysics.

1. Systematically one may argue for a physiological interpretation of Weber's law, as Müller did.¹⁷ Excitation varies somewhat as does the logarithm of the stimulus. No one doubts that excitation may have magnitude, and thus the quantity objection is met. Moreover the logarithmic relation between physical dependents is not unknown.¹⁸ Excitation, however, does not happen to be open to immediate observation, so we must observe its correlate sensation. We deal therefore with stimulus and sensation, which we must keep apart, avoiding the stimulus-error; and we escape from the formal objection that sensation does not have magnitude by making it a mere qualitative indicator of excitation which must have magnitude.

2. Wundt's psychological interpretation of Weber's law meets the quantity objection by the introduction of *Merklichkeitsgrade*. Sensations do not have magnitude, but if they did the matter would be irrelevant to psychophysics. It is apperception that gives a quantitative aspect to mind; there are degrees of noticeableness to sensations or to the differences between them.¹⁹ The sensation scarlet is not more than the sensation pink, but is more noticeable than the pink; and the difference between a scarlet and a pale pink is more noticeable than the difference between the scarlet and a rose. To introspection it is just as obvious that apperception has degree as it is obvious that sensation has not, and it is between these

¹⁷ G. E. Müller, *Zur Grundlegung der Psychophysik*, 1878, 224-403.

¹⁸ Müller, *loc. cit.*; Ward, *op. cit.*, 452-466; Titchener, *op. cit.*, II, ii, 66f. The autocatalytic theory is more recent: T. B. Robertson, *Monist*, 1909, 19, 372ff., 384f.

¹⁹ Wundt's theory passed through successive stages and no brief statement does it justice. For summary and discussion, see Titchener, *Exper. Psychol.*, II, ii, pp. lviff., lxxivf., lxxxvf., 69f.; for summary and genesis, p. lxxxii. note.

Merklichkeitsgrade and the physical values of the stimulus that the logarithmic law holds.

3. What was Fechner's answer to the quantity objection? To deny the stimulus-error, which the quantity objection implies. In the *Revision* he wrote:

"One must take care not to try to count relations that exist in the physical realm between physical units as existing within the mental province because they can be grasped only by the mind; for in so doing one loses the ground for distinction between the two provinces. Even the physical, within which the relations exist, must be grasped by our minds in order to exist for us and to be spoken about. Yet we discriminate on the basis of this community between outer and inner phenomena as between two provinces, and have to distinguish properties as belonging in the one or the other, not merged with each other or interchangeable, as might sometimes seem to be the case under a philosophical point of view. At any rate psychophysics takes this point of view and thus avoids confusion. Whenever something like a relation, a change, a difference, a unity, or a fusion, *etc.*, appears as characterizing the physical or psychical world, it is abstracted from the province of the one or the other, or it is counted into the one or the other province; it may occur just as readily in psychology as in natural science. Hence it is perfectly possible that the pitches should be represented in one province by something in the other province without our identifying the two. The relations of periodicity between vibrations, which occur as the psychophysical representatives of melodic and harmonic sensations, are the most obvious; the one is something very different from the other."²⁰

In other words it is all in our point of view. We can judge the stimuli or we can judge the sensations; and, according as we do the one or the other, we constitute for ourselves the physical or the mental world. It is nonsense to assume that, because we make judgments of physical phenomena in building up natural science, this natural science is a science of judgments and therefore mental. The two are distinct, and the discovery of the difference that is summarized by Weber's law attests the distinction.

To the writer of this paper it seems that Fechner's argument, turned a different way, becomes at least as invincible as any of the other ways out of the difficulty. The trick for escaping the force of the quantity objection, when directed against mental phenomena, is to turn it upon the stimuli themselves. Suppose sensations of weight do not under observation exhibit magnitude; what of the physical weights themselves? To physical observation ten grams is *a* weight and one gram is *a* weight; it is only in common sense, which is assuredly not physics, that ten grams is ten one-gram weights. Because physical phenomena, like mental, are referable to

²⁰ Fechner, *Revision der Hauptpunkte der Psychophysik*, 1882, 5f.

objects, is no excuse for reading the objects into them. Physical weight is as little the number of objects in the scale-pan as mental weight is the number of weights in the hand. The physical quantity is just as simple and unitary as the mental, and if sensation lacks magnitude so must stimulus. We can hardly, however, deny measurement to physics, and it thus appears that the quantity objection is not valid either against the measurement of sensation.²¹

4. Undoubtedly the most general way of meeting the quantity objection while saving mental measurement is by the substitution of the sense-distance for the sensation magnitude. Historically this conception dates from Delboeuf's *contraste sensible*. It is not necessarily incompatible with any of the foregoing accounts of mental measurement and is endorsed essentially by Wundt, Boas, Stumpf, Ebbinghaus, James, Meinong, Höfler, Stout, and G. E. Müller.²² It is the basis of Titchener's quantitative psychology, where it finds its clearest exposition.²³

This position holds that sensations, although they do not possess magnitude, may lie within a continuum, and that, although we can form no quantitative estimate of any sensation, we can nevertheless estimate the relative degree of separateness of two sensations within the continuum. Sensations are simply themselves and are not summed of various numbers of increments; the distances between these sensations, however, do vary and can be estimated in amount. The simplest case of mental measurement occurs when, for a series of three sensations, A, B, and C, occurring in a continuum, we estimate the sense-distance AB as equal to the sense-distance BC. Here we have measurement, for we have laid off the unit AB=BC twice in the distance AC, and it is the correlation of such estimated sense-distances with the corresponding values of stimulus that gives Weber's law.

²¹ The more thorough exposition would show that magnitude and measurement are systematic matters and are not found immediately at the observational level of science. The confusion is not unlike that of the systematic 'sensation' with the observational 'attribute:' cf. Sensation and system, *Am. J. Psychol.*, 1915, 26, 258-267, where Titchener makes this point. On the other hand, it is hardly fair to physics to say: "No sensation is a sum of sensation-parts or of sense-increments; no sensation is a measurable magnitude. Fechner has transferred to sensation a point of view that is right for stimulus, but that introspection refuses to recognize in psychology:" Titchener, *Exper. Psychol.*, II, i, p. xxvii.

²² Cf. Titchener, *op. cit.*, II, ii, p. cxxxiii.

²³ Titchener, *op. cit.*, II, i, pp. xxi-xxvii; ii, pp. cxvi-cxlv.

5. All the foregoing modes of meeting the quantity objection are successful without sacrificing the possibility of mental measurement; the fifth mode of defense consists in joining the enemy. We can give up the measurement of mind, substituting the measurement of sensitivity or of capacity-for-discrimination. Fullerton and Cattell give us our orientation here.

They declare, in the first place, that both sensation magnitudes and sense-distances are undiscoverable:

"If an observer can, in fact, estimate quantitative amounts of difference in sensation, apart from association with known quantitative differences in the stimuli, a relation between mental and physical intensity can be determined. The writers, however, agree in finding that they cannot estimate such quantitative differences in sensation in a satisfactory manner. We can indeed say when one weight seems approximately double another, but this is doubtless because we have often lifted first one volume, and then two, and the like. But we cannot say when one sound seems twice as loud, or one day twice as hot as another. We have made experiments to see how nearly different observers would agree in adjusting one shade of light midway between two others, and have found hesitation in coming to a decision and great divergence of opinion. Most men will think that a just king is happier than a tyrant, but few will agree with Plato in considering him 729 times as happy."²⁴

What is left? The observed *stimuli*²⁵ and errors of observation incurred in observing the stimuli.²⁶ There is no constant just noticeable difference nor threshold.²⁷ We have only errors of observation as we fail to observe an actual difference in the stimuli or, less often, observe a difference that is not there. These errors can be treated under the ordinary calculus of probabilities and follow the normal law of error.²⁸ The amount of the average error is always determinable and it increases with the magnitude of the stimulus.²⁹ It is the law of the dependence of the average error of observation upon the magnitude of the stimulus that Weber's law seeks to state, although the mathematical form of Weber's law is actually incorrect.³⁰

Such a quantitative psychology of error is of necessity a psychology of capacity,³¹—of the capacity of the organism to

²⁴ G. S. Fullerton and J. McK. Cattell, *On the Perception of Small Differences*, 1892, 20.

²⁵ Fullerton and Cattell, *op. cit.*, 9ff., 20, 153, etc.

²⁶ Fullerton and Cattell, 14ff.; Cattell, *Am. J. Psychol.*, 1893, 5, 287ff.

²⁷ F and C, 11, 150; Cattell, 288f.

²⁸ F and C, 12ff.; Cattell, 285ff.

²⁹ F and C, 23ff., 153f.; Cattell, 290ff.

³⁰ F and C, 24ff., 152.

³¹ Cf. Titchener, *Exper. Psychol.*, II, ii, p. cxxxiv note.

respond correctly to stimuli. This point of view has since developed into the statistics of mental tests and of behaviorism, which is a psychology of stimulus and response. It is a point of view for which there is no stimulus-error since quantitative judgments can be made only of the stimulus, and it is one that touches other sciences very closely since it concerns itself with observation, the method of every science.³²

The Two Psychologies

Thus it becomes evident that the answers to the quantity objection have divided along the traditional cleft in psychology. We have not only a psychology of datum and a psychology of capacity, but we have quantitative psychologies of datum and of capacity.³³

The quantitative psychology of datum ["the given"] insists upon a truly mental measurement. When further it correlates these mental measurements it is the true psychophysics. It may answer the quantity objection in any of the first four of the five foregoing ways, because any one of those four insists upon or explains the existence of mental quanta. It is a part of the larger psychology that is variously called structural or introspective, the psychology of process or of *Beschreibung*.

The quantitative psychology of capacity admits the quantity objection and denies—or at least ignores—mental quanta. This psychology sees no distinctively mental measurement, but undertakes the physical measurement of bodily response as a function of the physical quantities of the stimulus. There is no sharp epistemological line discernible between this sort of measurement and other physical measurement, and it thus meets the requirement of modern behaviorism that psychology interpenetrate physical science without sensible demarcation.³⁴ The psychology of capacity is also the psychology of mental tests³⁵ and of Urban's psychophysical experiments.³⁶ These

³² F and C, 9; Cattell, 285.

³³ Cf. O. Külpe, *Vorlesungen über Psychologie*, 1920, 5ff.; Titchener, *Am. J. Psychol.*, 1921, 32, 108-120.

³⁴ J. B. Watson, *Psychol. Rev.*, 1913, 20, 177; *J. Philos., etc.*, 1913, 10, 427; *Psychology*, 1920, vii.

³⁵ See note 31 above.

³⁶ The present writer has already had occasion to refer to F. M. Urban's position: E. G. Boring, *Am. J. Psychol.*, 1920, 31, 27f., esp. note 77; *Psychol. Rev.*, 1920, 27, 446f. It is well to repeat that no reference is intended here to Urban's later position, which involves an acceptance of the epistemology of Mach and Avenarius; see Ueber einige Begriffe und Aufgaben der Psychophysik, *Arch. f. d. ges. Psychol.*, 1913, 30, 113-152, esp. 113, 124f., with notes; cf. also *Am. J. Psychol.*, 1913, 24, 274.

latter seem strange mates, but the influence of Cattell is apparent in both. If the germ of the tests was in Galton, nevertheless it was Cattell, in the interests of the psychology of individual differences, who planted it in American soil where it has brought forth fruit abundantly. Urban's origin is less apparent. It is trivial to remark that he began his psychophysics in the laboratory that Cattell had founded years before. The evidence is internal and not explicit, but the seeker after information can find it.³⁷ In the broad, we may add, the psychology of capacity may become a functional psychology and deal, when it is experimental, with *meanings* and *Kundgaben* instead of mental processes and *Beschreibung*.

As a term the "stimulus-error" is the property of the psychology of datum. It characterizes the attitude of the psychology of capacity as being concerned observationally with the stimulus and admonishes against it. Of course the psychology of capacity ignores this admonition, for in making observations of the physical stimulus it is simply fulfilling its self-appointed task. The "stimulus-error" is no valid charge against this psychology, nor does it raise within it a question of right and wrong. No more can be expected of either psychology than that it hold to its premises.

In practice there is a difficulty, however. No matter how distinct the two psychologies may be, psychologists can scarcely hold strictly to the one or to the other. The psychologist of the datum can not be expected to attack a new perception without recourse to judgments of stimulus or of meaning,³⁸ and statements of meanings moreover may themselves become the object of psychological investigation.³⁹ As to whether the psychology of capacity might remain wholly faithful to the

³⁷ On psychophysical judgments and random events, compare Fullerton and Cattell, *op. cit.*, 12ff., 23ff., with Urban, *The Application of Statistical Methods to the Problems of Psychophysics*, 1908, 17f. On the meaning of the just perceptible difference, compare F and C, 11, and Cattell, *op. cit.*, 288f., with Urban, *Stat. Meth.*, 70. On mental measurement compare F and C, 20, 152f., and Cattell, 293, with Urban's ignoring of the issue in *Stat. Meth.* [Or could he have thought that Titchener had settled it for the sense-distance? Cf. *Psychol. Bull.*, 1912, 9, 245.] On judgment as directed upon stimulus, compare F and C, 20, and Cattell, 293, with Urban, *Stat. Meth.*, 5, 17, *Psychol. Rev.*, 1910, 17, 27ff. On recording degrees of assurance, compare F and C, 11, 151, with Urban, *Stat. Meth.*, 5ff. On the relation of psychophysics to physics, compare F and C, 151, Cattell, 285, with Urban, *Psychol. Rev.*, 17, 243f.

³⁸ Cf., e.g., L. B. Hoisington, On the non-visual perception of the length of lifted rods, *Am. J. Psychol.*, 1920, 31, 114-146.

³⁹ Cf., e.g., H. P. Weld, Meaning and process as distinguished by the reaction method, *Titchener Commemorative Volume*, 1917, 181-208.

stimulus it is not so easy to say. This attitude is more natural and it is possible to complete entire experiments without once trespassing upon the realm of mental process. On the other hand, the functional psychologies for all they have to say of the inadequacies of structural psychology, seem usually unable to complete the systematic mental picture alone, and a recent system of behaviorism has drawn unhesitatingly upon the psychology that it seeks to supplant.⁴⁰ In general what is fundamental to the one can not be ignored by the other, and on this account the writer of this paper would urge the attention of the psychologist of capacity to the "stimulus-error."

The Effect of the Stimulus-Error

If we are now to urge upon the psychology of capacity the avoidance of the stimulus-error, it is a fair demand that we state first the probable penalty that is incurred by a failure to accept our advice. Here we can not stand upon the epistemological ground that psychology observes mental processes and not stimuli, and that judgments of stimulus are therefore *a priori* inadmissible. This historical warning against the stimulus-error does not apply to the psychology of capacity which protests against a scientific dualism and deals by preference with stimulus and response. What we have to show is rather that the stimulus-error works against the establishment of the univocal correlations between stimulus and response that a psychology of capacity demands, that it interferes with the prediction of the response for a given stimulus. Here the ground is broadly scientific: we are dealing with the constancy of experimental conditions and the reproducibility of results.

When we go frankly to the literature, however, asking just what in numerical terms may be the effect in mental measurement of allowing judgments of the stimulus or of instituting them, we meet at first disappointment.

The psychology of datum is set to avoid, rather than to measure, the stimulus-error. It tells us where the stimulus-error is most insidious, *viz.*, in judgments of supraliminal sense distances.⁴¹ We may have trouble with the limens; we are almost sure to have it in comparing large sense-distances. Sometimes a special technique is necessary to avoid the error. In Martius' experiment on the apparent size of objects at different distances from the eye, all the stimulus habits for the estimation of the size of objects in everyday life are

⁴⁰ Watson, *Psychology, from the Standpoint of a Behaviorist*, 1919.

⁴¹ Cf., e.g., Titchener, *Textbook*, 218.

appealed to, and a special method is required "in removing the initial tendency of the observers to reflect on the actual size of the comparison rod in relation to the standard rod."⁴² In Angell's experiment on intensive distances between sounds one would expect concrete reference to the stimulus to enter much less readily; on the contrary, however, the observers tend to judge the height of fall or the angle of fall of the stimulus, and to neglect the mental datum.⁴³ For this error special remedies are presented. Müller urges that the amount of intensive difference be taken as the *Kohärenzgrad*, the *Leichtigkeit des Kollektivaufgefasstwerdens* of the two sounds.⁴⁴ Titchener suggests letting the observer blunder into the stimulus-error and then rescuing him by individualized treatment, his protocols in hand.⁴⁵ These experiments are striking cases, but even the psychologists who have no special measures of reform to offer cry out against the evils of judgments based upon secondary criteria, upon associates of the processes judged, or upon surrogate processes.⁴⁶ A surrogate, they complain, can even render incommensurables artificially commensurate, as seems to be the case when the intensities of weights and noises are compared in terms of *Spannungsempfindungen*.⁴⁷

Now it is not likely that there could be so much smoke without some fire; yet we are still at a loss to estimate the amount of danger. It would be reasonable for these psychologists of datum to seek to avoid stimulus for no other reason than that they are interested solely in "mind," but it is not to be supposed that the matter would have been taken so seriously had the quantitative results, and Weber's law which is dependent upon them, been unaffected by the kind of judgment given. Indeed this belief came to the surface when Grotenfelt, in defense of Weber's law, accused Merkel of the stimulus-error and inclined to the belief that results

⁴² G. Martius, Ueber die scheinbare Grösse der Gegenstände und ihre Beziehung zur Grösse der Netzhautbilder, *Philos. Stud.*, 1889, 5, 601-617, esp. 605f.; cf. Titchener, *Exper. Psychol.* II, ii, pp. 262f.

⁴³ F. Angell, Untersuchungen über die Schätzung von Schallintensitäten nach der Methode der mittleren Abstufungen, *Philos. Stud.*, 1891, 7, 414-468, esp. 438.

⁴⁴ G. E. Müller, *Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik*, 1904, 237f.

⁴⁵ Titchener, *Exper. Psychol.*, II, ii, p. 198; cf. also pp. 203f., 230.

⁴⁶ Cf. Fechner, *Elemente*, II, 318ff.; H. Neiglick, *Philos. Stud.*, 1888, 4, 41; Angell, *op. cit.*, 438; W. Ament, *ibid.*, 1900, 16, 173; G. E. Müller, *op. cit.*, 241; J. Fröbes, *Z. f. Psychol.*, 1904, 36, 259.

⁴⁷ H. Münsterberg, *Beiträge zur experimentellen Psychologie*, III, 1900, 56-122, esp. 98ff.

that followed Merkel's law might possibly indicate that they were based upon judgments of stimulus.⁴⁸ The data that we wish, however,—the comparison in quantitative terms of results of judgments of process—are, in the earlier history of psychophysics, lacking.

(The psychologists of capacity, we may note in passing, are not to be asked for this comparison. Cattell and Fullerton denied the possibility of mental measurement. They can not therefore be asked for its comparison with any form of physical measurement.)

On the basis of recent literature, however, there is something to be said, in answering this question, for the case of lifted weights, and very much to be said for the case of the limen of dual cutaneous impression. We may mention the lifted weights at once and reserve the two-point limen for the next section.

Friedländer undertook a comparative study of lifting weights under different *Einstellungen*.⁴⁹ He employed a "*G-Einstellung*" in which the attention was directed upon the lifted object (*Gegenstand*, hence "*G*"), and an "*A-Einstellung*" in which the object was abstracted from (hence "*A*") and the attention directed upon the sensory aspect of the experience (*Druck-, Spannungs-, Kraftempfindungen*).⁵⁰ Here we should expect to find the results we are seeking, for the *G-Einstellung* is the stimulus-attitude, the attitude demanded by a psychology of capacity and called the "stimulus-error" by the psychology of datum; and the *A-Einstellung* in its various forms is the process-attitude which avoids the "stimulus-error." There is not the least doubt that the two attitudes give different results. "Differential sensitivity on the whole is somewhat finer for the G-series" for a standard weight of 500 g., Friedländer tells us, but a standard of 1200 g. may give a finer discrimination under the *A-Einstellung*. The data unfortunately are for one observer only. They are based on too few cases,—41 series after the practice-effect was presumably constant. The observer did not always succeed in maintaining the required attitude, for the stimulus-attitude was difficult for him (!) and he sought to make his finer discriminations under the *A-Einstellung*. The resultant psychometric functions are not smooth ogives; one just barely misses inversion in its central

⁴⁸ A. Grotenfelt, *Das Webersche Gesetz und die psychische Relativität*, 1888, 111f.; cf. Titchener, *Exper. Psychol.*, II, ii, pp. lxxviii f., 219.

⁴⁹ H. Friedländer, *Die Wahrnehmung der Schwere*, *Z. f. Psychol.*, 1920, 83, 129-210, esp. 187-193.

⁵⁰ Pp. 133ff.

portion.⁵¹ We are not yet ready, then, to generalize as to the exact effect of attending to the stimulus in making psychophysical judgments. All we can say is that there is an effect, that a shift in the observational attitude alters the numerical results significantly. We shall not dare, therefore, if we wish to predict response from stimulus, to leave attitude out of account.

My colleague, Professor Fernberger, has recently completed similar experiments in the Clark Laboratory. He had three observers, and extended his series through many fractions to take account of progressive practice and to give an adequate number of cases. His resultant ogives are smooth and regular, and his procedure seemed calculated to yield all that could be desired methodically. He gets differences for the different attitudes,—more striking differences in some cases than Friedländer's. More than this I can not say in advance of the publication of his results. Perhaps in his final analysis he will discover a generalization, which is not apparent to casual inspection of the functions and constants. The results indicate unquestionably that an alteration of attitude by instruction may result in an alteration of the psychometric functions, which is significant in the mathematical sense of being many times its probable error, but which for a given observer is unfortunately quite unpredictable. Attitude may be very important even when we can not say just why.

Fortunately the case of the two-point limen is less mysterious.

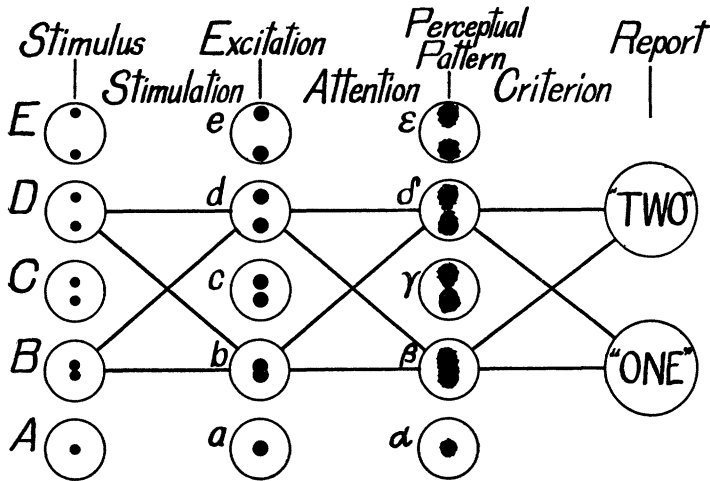
The Stimulus-Error as Equivocal Correlation

The limen of dual impression upon the skin furnishes the case for which we are looking. We know, not only that judgments of stimulus may here make a difference in the quantitative results, but we know further how great this difference may be and something of its conditions. We are in a position, moreover, to generalize from these facts with some assurance and to assert that the effect of the "stimulus-error,"

⁵¹ In fact it is not even clear that Friedländer's cautious generalization is not in part an artifact. If we compute the data of Table 10 by Urban's procedure for the *Konstanzmethode*, we get:

	A-Einstellung	G-Einstellung
Av. measure of precision (h)	0.0138	0.0147
Interval of uncertainty (grams)	35.39	37.13
Point of subjective equality (grams)	500.8	509.9

There is not much difference in precision or discrimination by this method. The striking difference is in the effect of attitude on the point of subjective equality.



from the point of view of a psychology of capacity, is—under similar conditions, at least—to render the correlations between stimulus and response equivocal and thus to jeopardize the rigor of conclusion that science demands.

We may proceed to the point by reference to the visual schema of the figure reproduced herewith. The diagram is intended merely to assist in the analysis of the factors involved and not as an actual picture of neural or psychophysical fact.

One deals in determining the two-point limen with a series of stimuli, *A, B, C, D, E*, pairs of stimulus-points at different separations, with perhaps a single point, *A*, at the extreme of the series. From the work of numerous investigators⁵² we know that there is a similar series of perceptual patterns, *α, β, γ, δ, ε*, which passes, with approximate regularity, from a sharp point to a blunt point, to an oval, to an elongated oval, to a double-paddle, to a dumb-bell, to two separated points. We may best think of these perceptual patterns as the process material of the psychology of datum, but they exist also for the psychology of capacity as inscrutable "middle terms." Intermediate between stimulus and process we are accustomed to assume some sort of excitatory process, *a, b, c, d, e*. Of

⁵² G. A. Tawney, *Psychol. Rev.*, 1895, 2, 585-593; V. Henri, *Ueber die Raumwahrnehmungen des Tastsinnes*, 1898, 6; M. Foucault, *L'illusion paradoxale et le seuil de Weber*, 1910, 122-145; E. J. Gates, *Am. J. Psychol.*, 1915, 26, 152-157; and numerous other references cited in these articles.

these there must also be a series, and knowing little about them, we may picture them as somewhat like the perceptual processes. Finally there is the verbal report which is generally limited experimentally to the words *One* and *Two*. It seems, moreover, that we keep within the limits of scientific good sense if we say that stimulus, excitation, mental process, and report form a dependent series, and that each is the resultant of the preceding.

If now we choose for special consideration the stimuli *B* and *D*, we may note that they most frequently, perhaps, give rise to the dependent series '*B-b-β-One*' and '*D-d-δ-Two*,' but we must note further that cross-connections are possible.

Stimulus gives rise to excitation by way of stimulation. Here variation may tend either in the direction of fusion or of separation. On the forearm, for example, the angular shifting of stimulus toward the longitudinal axis may result in fusion and *D* may give rise to *b*; if the rotation is toward the transverse axis, *d* may follow upon *B*.⁵³ Location as well as angular orientation also yields these differences. Near the elbow *D* gives *b*, say; a little further down *B* may give *d*.⁵⁴ It is also possible that the chance impingement of the stimulus-points upon multiply innervated spots in the skin gives rise to multiple excitation (*B* to *d*) and that the stimulation of less complexly innervated spots may lead to a simpler excitation (*D* to *b*).⁵⁵

At the next phase of our series excitation culminates in perceptual pattern under the selective action of attention. These terms sound strange of course to the psychologist of capacity, but we may ask him to accept them or to find substitutes for them. They stand for factors that affect the correlations with which he is working, and he must take scientific account of them, whatever he calls them.

It is experimentally demonstrable that under inattention potential *Twos* become *Ones*; one stimulating point or the other catches the attention and thus withdraws it, as it were, from the other.⁵⁶ The converse occurs in the well-known *Vexirfehler*, the paradoxical judgment, where two points close together or even a single point give a perception of duality. Henri and Tawney referred this phenomenon in part to attention, and its dependence upon certain suggestive factors

⁵³ Cf. Tawney, *Philos. Stud.*, 1897, 13, 170; cf. Boring, *Quart. J. Exp. Physiol.*, 1916, 10, 23.

⁵⁴ Cf. Henri, *op. cit.*, 26f.; cf. Boring, *loc. cit.*

⁵⁵ Henri, *op. cit.*, 64ff.; Boring, *op. cit.*, 89-93, esp. 93.

⁵⁶ Cf. A. Brückner, *Z. f. Psychol.*, 1901, 26, 54f., 60; Boring, *op. cit.*, 88f.

seems to indicate that it is of this order.⁵⁷ Griesbach and others supposed that fatigue reduced the limen,⁵⁸ but Friedline has shown that the effect of fatigue is operative only among the perceptual forms at the lower end of the series, say α and β . Here fatigue acts in the direction of fusion and may perhaps be another factor that operates within the attentive phase.⁵⁹ Thus the perceptual pattern is not wholly dependent upon conditions at the periphery, the mode and place and nature of the stimulation. Certain more central factors also come into play and justify us in adding to the cross-connections $B-d$ and $D-b$ the connections $b-\delta$ and $d-\beta$.

Finally now the perceptual pattern issues in a judgment in accordance with criteria of judgment that have been established. The available factor here lies in the setting up and the preservation of these criteria. Left to himself there is no guarantee of what the subject will do, as the studies of Titchener,⁶⁰ deLaski,⁶¹ and Friedline⁶² all show. Perhaps the most normal criteria for sophisticated adult subjects would be such that the perceptual δ of our chart would lead to the judgment *Two* and the perceptual β to the judgment *One*. It is a reasonable inference that McDougall's savages in Torres Straits, under the competitive incentive to do well, assumed such criteria that even our β would have led to the judgment *Two* for plainly β is larger than the pattern which a single point most often gives.⁶³ An overzealous subject can interpret perceptual patterns lying very low in the series as meaning the presence of two points upon the skin; in fact, as Friedline has shown, he can do amazingly well under the influence of both practice and incentive, provided he is not fatigued.⁶⁴ On the other hand the sophisticated subjects of the laboratory, and, it may be, therefore the Englishmen, whom McDougall compared with the savages, tend to define *Two* by reference

⁵⁷ Henri and Tawney, *Philos. Stud.*, 1895, 11, 394-405, esp. 403ff.; Tawney, *ibid.*, 1897, 13, 186-198; Henri, *Raumwahrnehmungen*, 61-66.

⁵⁸ H. Griesbach, *Arch. f. Hygiene*, 1895, 24, 124ff.; but see the summary in C. L. Friedline, The discrimination of cutaneous patterns below the two-point limen, *Am. J. Psychol.*, 1918, 29, 415-418.

⁵⁹ Friedline, *op. cit.*, 411f., 418f.

⁶⁰ Titchener, On ethnological tests of sensation, etc., *Proc. Am. Philos. Soc.*, 1916, 55, 206-215.

⁶¹ E. deLaski, On perceptive forms below the level of the two-point limen, *Am. J. Psychol.*, 1916, 27, 569-571.

⁶² Friedline, *op. cit.*, 405-415, esp. 405f., 408f., 411f.

⁶³ W. McDougall, *Rep. Cambridge Anthropol. Expedition to Torres Straits*, 1903, II, 189-193; cf. Titchener, *op. cit.*

⁶⁴ Friedline, *op. cit.*, 408f., 414f.

to a perceptual pattern higher in the scale.⁶⁵ We should hardly have been bold enough to have predicted this result; to the writer, however, it seems reasonable enough now that it has been pointed out. Is it not to be expected that the savage would try to "do well" by discriminating as finely as possible and that the sophisticated person would try to "do well" by discriminating as accurately and consistently as possible, though less finely? In any case the point is that the acceptance of a criterion is an unavoidable experimental condition in determinations of cutaneous spatial sensitivity, and that the criterion must therefore be controlled, since when uncontrolled it gives uncertain significance to the verbal responses *Two* and *One*. In fact it appears that the apparent limen for cutaneous duality may be very much more than quartered by a variation of criterion, and it seems further that the conflicting results in the literature with respect to the effect of fatigue and the effect of practice upon the limen are to be explained in this way.

This leads to a conclusion. If only the end-terms of stimulus and response are controlled a univocal one-to-one correlation between stimulus and response is not possible. In the terms of the diagram, both the stimuli *D* and *B* may condition the response *Two*. There are eight paths leading to *Two*, four from *D* and four from *B*. If we consider the other factors of the schema, the situation is enormously complicated. There are over a hundred modes of connection from *A*, *B*, *C*, *D*, and *E*. to *Two*. Certainly the actual possibilities must be legion. At this level of work the best we can do is to remain in the dark and to deal with relative frequencies, yet relative frequencies do not yield the predictive correlation that science demands. The only way to get out of the dark would be to study the effect of stimulation, of attention, and of criterion by taking hold of these dependent series at their intermediate points, thus providing ourselves with a more complete knowledge and control of the entire psychophysical situation. Now the psychologist of capacity habitually controls stimulation, the various adjustments of the stimulus to the sense-organ, but the psychologist of datum also controls by instruction both attention and criterion. He does not, to be sure, reach the ideal of 100% certainty in the prediction of the response to

⁶⁵ McDougall, *op. cit.*, 192 and note; Titchener, *op. cit.*, 211.

a given stimulus, but he is able greatly to increase the precision of these stimulus-and-response correlations. On the other hand, the failure to control the attitudinal factor implied in the acceptance of a criterion, and the attentional factor, again and again results perforce in an equivocal determination of these responses, which is nothing more nor less than a "stimulus-error."

In the psychology of capacity, then, the danger of the "stimulus-error" reduces to the danger that judgments of stimulus will prove scientifically equivocal. Experimentation will show when they are and when they are not. When they are equivocal, the problem is soluble by refinement in the control of conditions. The modern technique for the control of attention and attitude is a method that satisfies scientific standards of accuracy of prediction better than any available substitute. To shut our eyes to this technique in the absence of a substitute would be to refuse to accept scientific methods that have already yielded practical results. Certainly if the psychologist of capacity is to be a successful experimenter, joining hands with the biologist and physicist, he must in some way take account of all the means of experimental control that have been demonstrated as essential to the securing of accuracy.

Summary

Scientific psychology in its inception assumed a distinction between mind and matter and the separate existence of observable mental data and observable physical data. Fechner's psychophysics sought to measure the mental data and to establish their correlation with related physical data. The opponents of this point of view raised the *quantity objection*, arguing that mind is not possessed of magnitude and is therefore not measurable. Most of these objectors were attacking only the quantitative status of psychology and seeking to establish it as an essentially qualitative, but mental, science. Other objectors preferred, however, to keep psychology quantitative by conceiving of it as physical, as the psychology of the *capacity* of the organism for response to stimulus. The older psychology met the quantity objection by showing that the nonexistence of mental magnitude does not preclude mental measurement, and then sought to protect itself against incursions of the physical observational attitude of the psychology

of capacity by styling that attitude the *stimulus-error*. The implication would be that a psychology of capacity does not need to avoid the stimulus-error, but rather should cultivate it. The thesis of this paper is, however, that recent researches have shown that the observational attitude which is directed upon the stimulus—the attitude of the stimulus-error—may sometimes lead to equivocal correlations of stimulus and response which, because equivocal, are unscientific. In the case of the limen of dual impression upon the skin, for example, a psychology of capacity must make use of introspective data if it is to attain its own ideals.